Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

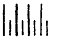
JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

sitic Algæ. He also said the spicules of the sponges in the Niagara River are sometimes cemented together by a kind of hard chitinous matter, which preserves the shape of the sponge. I have not determined its exact nature. It does not seem to be calcareous, and is not attacked by ordinary acids or alkalies.

"LIGHT AND ILLUMINATION," by Ernst Gundlach (Page 79).

Dr. Lester Curtis said, One point of great interest referred to in the paper is that relating to the research of Prof. Abbe and the important results he has reached in the study of diffraction and its part in illumination and microscopic vision. These studies may be familiar to many of those present, but are probably not so to all. The doctor then proceeded to give a *résumé* of Abbe's discoveries, illustrating his remarks by the blackboard.

If the eye-piece be removed when we are using a high-angled objective on a fine-lined object like *Pl. angulatum,* we shall see a central image of the flame, and also six colored spectra surrounding it. If the objective is just able to resolve *angulatum* the spectra will be violet-colored. If it be a modern high-angled glass, other spectra outside of these six will be seen, and if other colors following violet in the reversed spectra, such objectives are able to resolve more difficult tests than *angulatum,* and to do so by reason of these very spectra. In order to resolve any fine-lined or striated object, it is absolutely necessary to get these spectra, and these will be distant from the central image exactly in proportion to the fineness of the lines in the lined object, being closer to the center as the lines are coarser; farther away as the striæ are finer. When everything is favorable in the other conditions, the limit of the resolvability of an objective will be reached when the lines are so fine in the object that the outer spectra which they form are just visible at the extreme edge of the visible field of the objective. If the spectra fall beyond this, the lines cannot be resolved by that objective. But now, in this latter case, if the mirror be moved so that the light is no longer central but falls obliquely, from say the right side, the spectra will begin to appear on the other, or left side of the field of the objective, and the lines are now resolvable. So that the real limit of resolvability of any objective is reached when the outermost spectra formed by the lines of the object are just visible in the outer edge of the field of the objective when the light falls from the opposite side at the extreme angle which that objective is capable of receiving; and when an objective can be made that is capable of receiving the full possible obliquity of light, viz., 180° in glass, it reaches the limit of resolution. These, now, are Abbe's discoveries in brief, and too, these are facts that are easily verified by any skillful microscopist. Abbe's theory of the cause of the appearance is as follows: When the light passes up through any fine-lined object, only part of the light gets through and reaches the objective, and this is made to deviate from its course at an angle depending on the fineness of the lines. Thus, these different kinds of light overlap each other and produce those belts of color and light and shade that are known as diffraction phenomena. The phenomena in this case, however, are not the same as ordinary diffraction.(?) The closer the lines the greater is this angle of deviation, and so more than one pair of spectra may be produced. If now the objective is able to take in and recombine all these spectra, it will produce a clear image of the lines; if it cannot do so, it will produce an image differing in many ways from the real nature of the object. The value of aperture is thus not that it admits a so much larger cone of light, as that it is able to receive and to recombine more of these diffraction spectra. The theoretical limit of resolvability, according to Abbe, will be when the lines are only two wave-lengths of light apart.

Now the truth of this great theory—one of the most remarkable discoveries of optics—is easily demonstrated by the sure facts of experiment. If we look down the tube at the back lens of an objective focused on a plate having two sets of fine lines ruled thus,  the lower half ruled twice as close as the upper, we shall see two sets of spectra, the finer set above produced by the coarser lines, and a coarser set below produced by the finer lines. (Coarser, that is because spread wider apart from each other by the finer lines.) Now, by taking some pieces of paste-board, cut to fit the upper end of the objective, and cutting slits in these, we can make diaphragms of different shapes by which we can cut off part of these spectral images. If we cut off all the spectra, leaving the central image, we shall have a dioptric image of the figure on the plate, if there is any, without any lines. If we cut off the inner spectra we shall have visible a field covered over with lines of double their real fineness. If we cut off the first and second sets of spectra on each side of the central image we shall see, through the eye-piece, the field, covered with lines of quadruple, the real fineness. So that the image of the lines is entirely produced by diffraction, and is not a dioptric image at all, and is only correct when the objective receives and recombines all the diffractive spectra. If a part only are used, the image will be different from the object.

Dr. Mercer.—We all know that sound-waves can travel around a corner; so will light pass around a corner, if the edge is not more than a small multiple of a wave-length of light, and thus will produce the phenomena called diffraction. Abbe distinguishes two kinds of microscopic vision; one like ordinary vision, by rays directly from the object refracted to the eye, which he calls the dioptric image. The other is produced by fine-lined or very minute objects, so fine as to be but a small multiple of the wave-length, and called the diffractive image formed by the recomposition of the spectra produced by these fine edges or lines, which when less than the $\frac{1}{3000}$ of an inch, can only produce an image (and be seen) by means of these spectra.

Some of Abbe's pupils, it is said, have taken up these questions and worked them out very fully as theses, and have thus shown the truth of the theory, having been able to predict, from mathematical calculations, what appearances should be produced by certain conditions prescribed in the problem as announced by Abbe, but not seen by them; and these appearances have been afterwards verified by observation. One case was very interesting. The professor gave to the pupil to calculate the optical image produced by a series of rows of knobs at a certain distance and angle, being, in fact, the markings of *angulatum*. The student worked out the problem, described the effects which ought to be produced, among which were the manipulations of a series of small triangular markings. These had not before been observed, but, on examining a frustule of *an-*

gulatum under the conditions named in the problem, the small triangles were seen.

Dr. Lucy asked, Why it was that, when examining a fly's eye, one can get, in one part of the field, a hexagonal appearance of the cells, and in another part it is not so seen at all? Is it on account of the illumination, or what?

Dr. Mercer.—By Abbe's series of diaphragms you can change dots into lines, or hexagons, or squares, or *vice versa*,—make lines seem closer apart than they really are.

Dr. Fell.—These different appearances to which Dr. Lucy refers, are partly the result of perspective. We do not get the hemispherical eyes of insects perfectly flattened out, and, hence, their appearance is often deceptive.

President Blackham.—As to the triangle, so-called, in the *P. angulatum*, he was not aware that these were new, as would seem to be implied from the experiment of Abbe just referred to by Dr. Mercer. He had, himself, long been familiar with them. Nachet's well-known illustration of this optical illusion well shows how easy it is to perceive hexagons where the bodies are really round dots.

Dr. Mercer said, What Abbe referred to was not the triangular spaces which seem to be seen between the hexagonal-appearing beads, but small triangular dots within these spaces. These he believed had not before been noticed.

Dr. ——— said, These dots are explained by the Abbe diffraction principles just explained by Dr. Curtis. The question of the truth or falsity of Abbe's theory is one of the most vitally important questions in microscopical science to-day. Not only the line of our further advancement, but the reality of what we now see, is in question. And yet, by the very terms of the theory, its absolute proof is impossible; for, if the image is dioptric up to a certain point, and then becomes a diffractive image, how can we tell whether the image is real, or only diffractive? Once Dr. Taylor and myself thought we had discovered a ground of difference between these images, but further study convinced us we were mistaken.

——— said, The diaphragm experiments given by Abbe, and already described here, are the proof of his theory. They are a demonstration of the whole matter. The central image seen in looking down the draw-tube is a dioptric image, and this shows only its outline and coarser features. If your objective can utilize only this central light, you will get only this dioptric image, and see none of the finer details of the object. If your glass can utilize the spectral lights on either side of this central image, then you get a diffraction image upon the dioptric one, revealing the finer details as well as the main outlines. If you cut off the central image and use only the side images, you will have the diffraction image only, and can then see the fine lines, but not the outlines of the object.

Mr. Vorce asked, How it was true that the image can be seen by dioptric vision when the lines are closer than one two-thousandth of an inch.

Dr. Curtis explained that Abbe's theory was, and experiments proved, that the dioptric image was only formed when the lines were coarser than one two-thousandth of an inch. If they were finer than that the image seen was purely a diffraction image.

Dr. Krackowizer, referring briefly to the points brought out thus far said, Nothing was easier than to find how liable we were to error in macroscopic or ordinary vision, and in regard to good-sized objects. He would give a typographical instance, to draw from matter he was specially familiar with as a member of the press. To the ordinary eye the two loops in a capital **B** or an **8**, appear of the same size, but that they are not, any one can easily convince himself by turning it upside down when it is evident that the lower loop of the character is much the largest, and must be so in order to have them seem even and symmetrical in the ordinary letter. Again, draw two parallel lines, and draw over them a set of lighter parallel lines oblique to the first. The original pair will no longer seem parallel. Other examples, more or less familiar to them all, might be added. from which he concluded that the chief part of the trouble with cases of imperfect microscopic images of objects lay in our own imperfect vision.

Dr. S. O. Gleason said, The great question after all is, what do we really see, and what is the certitude of microscopic vision. We have a glass plate said to be ruled one hundred and fifty thousand lines to the inch, and the makers tell us they can demonstrate the fact mathematically. Now if I put this on the stage of the microscope and look at the first or coarser band I see them easily. I continue to look for the other lines, and moving the plate along I see the other bands successively, the lines getting evidently finer and finer as they are claimed to be, and requiring more and more oblique light till I go through the entire plate apparently, and seem to see these finest lines which are beyond the boundary of theoretical visibility according to the theory of which we have been hearing; the question recurs *do* we see them or do we not? Do we see what we don't see, or don't we see what we do see? (Laughter and applause,)

President Blackham read a part of the table published by Mr. Crisp in the covers of the *Journal of the Royal Microscopical Society*, calling attention to the theoretical limit there laid down for each class of objectives: dry, water immersion, and homogeneous immersion, according to which the utmost limit of resolvability for a *perfect* crown-glass-front objective of highest possible angle would be lines 146,528 to the inch, with the green ray. Now, Abbe's theory stands or falls with the truth or falsity of this limit. If lines finer than 147,000 to the inch can be seen and counted, or satisfactorily shown with a crown-glass-front objective, then Abbe's theory falls to the ground. I do not like to say it *has* been done, but I am strongly inclined to suspect so. I am free to confess I do not attach the value to Abbe's theory that some do. He is doubtless a great mathematician. So was Newton, and yet Newton was entirely mistaken as to the possibility of making achromatic lenses, and Abbe may prove to be mistaken also. I hope he may be, for, if he be not, then we are already nearly at the limit of any further advance in the improvement of objectives.

Dr. T. S. Up de Graff said, It has been said that an Englishman once complained that no British scientific man could get up a theory but straightway some impudent Yankee would proceed to upset it by some new patent invention. *Perhaps* this great German theory would share the same fate, and the wonderful objectives of Bausch & Lomb, of Gundlach, of Tolles, and of Spencer may prove the very inventions to do it. The first question, however, was to be sure that the lines were really ruled to the fineness claimed. He would like to hear from Mr. Fasoldt.

Mr. E. C. Fasoldt said, That the closeness to which the lines were ruled was settled by the graduation on their ruling machine, and could also be shown by a delicate pair of counting micrometers for stage and for eye-piece that they had constructed. He was assured by Dr. Woodward that the standard meter used when they constructed their ruling machine was now known to be slightly in error, so that the millimeter used was a little short, and therefore on this nineteenth band there were *more* than 152,000 lines to the inch. [Laughter and applause]

"EFFECTS OF DIVISION OF THE VAGI OF THE HEART," by Dr. Bleile and A. Feiel (Page 91).

Dr. Geo. E. Fell said, The paper was one he desired to commend. It was a step in the right direction. The changes produced in tissues through sections of the nerves which control them, is an important subject for consideration. In the starting point of pathology, a defect in nerve nutrition or action may frequently produce the lesion—Dr. Bleile's experiment may simulate, in effect, abnormal conditions, and he hoped they would be followed up by the authors and by other members.

Dr. Krackowizer said, The paper was exceedingly interesting, and, happily, so free from abstruse technicalities as to be followed by others than specialists in their particular line of work. Does Prof. Bleile say, that in the case of those animals which did not die of inanition he discards entirely the theory of degradation of tissue ? Or does he assume that the cutting of both vagi would produce a total degeneration of tissue ?

Dr. Bleile.—No. In the experiments of Eichorst, all on pigeons, total degeneration was produced. We experimented only on rabbits; not on invertebrates. Our rabbits all survived, apparently for an indefinite time, one living thirty-two days, and dying then by accident. The change comes on after one day's division of both vagi, though evidently not so marked as Eichorst says. After the lapse of seven days the granules are probably fatty.

Dr. W. C. Barrett said, This question was one of absorbing interest to every medical man. The great type of disease at the